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Operations Research in the Next 20 Years: A Technological Forecast

METHODOLOGICAL RESEARCH DIVISION
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Operations Research in the Next 20 Years: A Technological Forecast

by

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FOREWORD

The paper published herewith was presented at the Eighth ABCA Discussions on Army Operational Research, held at the Industrial College of the Armed Forces, Ft Lesley J. McNair, Washington, D. C., 17–28 June 1963. It is included in the list of titles below representing RAC papers that were prepared originally for the Eighth Quadripartite.

Lynn H. Rumbaugh, "Critical Appraisal Address, United States," RAC-TP-102. UNCLASSIFIED

Nicholas M. Smith, "Operations Research in the Next 20 Years: A Technological Forecast," RAC-TP-103. UNCLASSIFIED

Philip H. Lowry, "Dual Capability in a Theater of Operations (U)," RAC-TP-104. SECRET

C. A. Warner, "Southeast Asian Conflict and Operations Research (U)," RAC-TP-105. CONFIDENTIAL

Irving H. Siegel, "Allocation of Resources: Problems of Strategic Posture and Readiness," RAC-TP-106. UNCLASSIFIED

H. N. Hoppes, "Promoting Combat Developments through Mathematical Models," RAC-TP-107. UNCLASSIFIED

Anthony V. Fiacco and Garth P. McCormick, "Algorithm for Nonlinear Programming: Resource-Allocation Methodology," RAC-TP-108. UNCLASSIFIED

James W. Johnson and Richard E. Zimmerman, "A Quick-Gaming Calculation of Seventh Army Defensive Operations (U)," RAC-TP-109. CONFIDENTIAL

PREFACE

This forecast was prepared in response to a request from the Office, Chief of Research and Development (OCRD) for a contribution to be incorporated in "FY 1963 Long-Range Technological Forecast," prepared annually by the Department of the Army.

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Operations Research
in the Next 20 Years:
A Technological Forecast

ABSTRACT

A long-range (0 to 20 years) forecast is attempted in the development of the methodology and techniques of operations research. The forecast deals with expected advances in techniques of decision procedures, particularly in the realm of value and policy analysis. Progress in methodology is more greatly needed than progress in computer engineering. The need for, and estimated progress in, the development of macromodel theory is described.

INTRODUCTION

PROBLEM

To forecast the scientific advances of the next 20 years in the techniques of operations research.

SCOPE

A forecast in the problem area was prepared, as of the spring of 1963, as a 20-year projection. Time periods are described as "short range" (0 to 4 years), "midrange" (4 to 8 years), "long range" (8 to 12 years), and "very long range" (12 to 20 years). The procedure for preparing the forecast is:

(a) Introduction of technical definitions relating to operations research.

(b) Compilation of the types of (Army) management problems now needing solution.

(c) Analysis of these problems with respect to the techniques called on.

(d) Prognosis of advances in these techniques.

(e) Examination of the technical development forecast for other sciences that may contribute to operations research.

(f) Forecast of progress in solutions of the original set of problems.

DEFINITIONS

Operations Research

This applied professional service rendered to a responsible commander or manager client as an aid to, or recommendation of, the client's decision in a specified problem area—in terms of the client's goals and policies and by a "method" that brings to bear all the applicable processes of logic, mathematics, science, and axiology—purports to lead to decisions that produce an overall increase in the probability of attaining the client's goals, or more abundant rewards, and/or a decrease in the overall consumption of resources.

Axiology

As used here, axiology is defined as a system of evaluation—the set of values, norms, and policies pertinent to a given decision area, together with the method of warranting such values, norms, and policies and the process of modification of these quantities.

Techniques of Operations Research

These elements of the previously mentioned systematic method will be detailed as the need to discuss them arises. In general, the techniques can be divided into four broad classes:

Decision Algorithms. The application of a decision principle as a prescribed set of operations, an example of which is the "maximize" or "minimize" directive, i.e., "Minimize the expected cost of development and production of a weapons system designed to meet these specifications . . . ." Such decision directives are always accompanied by a set of restraints as to permissible quantities of human and material resources, other physical restraints of the environment, and also policy restraints. In decision-algorithm problems the objectives of the situation are given, the restraints and resources are known, and the measures of values or effectiveness are known. The requirement for techniques arises from the complications introduced by multiple goals, numbers and types of resources and restraints, and their combinatory—or logical—relations, and from other complications involved in probabilistic situations and time-dependent elements. Also included among techniques in this class are mathematical developments in optimization theory.

Value and Policy Determinations. Techniques in this class include determination of measures of effectiveness as determined by, or related to, the overall objectives of the commander or executive; determination of the values of such specific resources as human life, materiel, and strategic commodities; determination of the relative long-range value of specific tactical and strategic doctrine; analysis of policy as a set of general-decision strategies; measurement of human stress and morale; and the relation of military plans and readiness to national goals and objectives.

Organizational and Systemic Measures and Techniques. These techniques apply to problems relating to organization as a hierarchy of command and control echelons and to the design and/or analysis of command structure, communication links, time factors, characteristic systems response, logistics, control doctrine, perturbations introduced by weapons or other technological or tactical innovations, perturbations resulting from combat casualties and/or replacement by less-experienced elements, training, processing of intelligence information, retrieval of information, etc.

Techniques Relating to the Establishment of Warrantable and Practicable Decision Models. Comprising the epitome of the method by which operations analysis claim to improve the decision process, these techniques lead to criteria for the admissibility and warrantability of the models used with respect to the class of decision problems to which they are applied. The problem in development of this class of techniques is twofold: conceptual problems relating to the combination of logic, intuition, and experience that can apply to the problematic situation (i.e., concept formation or "objectification" of an applicable model in the first place) and development of applicable models and computational techniques to produce practicable models (i.e., decision models that can be developed and exploited with an admissible—and profitable—expenditure of resources and in time). This class of techniques includes development in simulation and gaming techniques and the like, as well as in associated sciences: computer development, display and ancillary computer-equipment development, and computer-programming development. Advances in these fields presuppose certain conceptual developments and reconstructions at the level of philosophical foundations of the cognitive act itself in an area loosely defined as "cognitive theory" and "philosophy of science."

PROBLEMS AND TECHNIQUES

PROBLEM AREAS OF ARMY COMMAND AND MANAGEMENT

At present, operations research is deficient in application to the following types of problem areas of Army command and management:

(a) Allocation of military resources and budgets and determination of military costs.

(b) Management and budgeting of research and development (R&D) programs.

(c) Analyses (measures) of military effectiveness.

(d) Projection of technological and tactical innovation into determination of the future battlefield.

(e) Design of experiments and/or simulation for the production of data applicable to future battle conditions.

(f) Design of admissible, meaningful, and practicable models leading to decision principles under uncertainty, particularly uncertainties that increase with projection into the future.

(g) Extension of practicable models to more complex or more comprehensive problems, in particular the inclusion of a more complex and realistic set of environmental conditions.

(h) Improvement in methods of filtering, processing, and retrieving information.

(i) Connection of military-effectiveness models with logistic and economic variables.

(j) Analysis and diagnosis of deficiencies of management policy.

(k) Conceptualization of warrantable macromodels of combat.

The management areas may be divided into two classes: those for which there is as yet no well-formed conceptual method of solution and those for which conceptual methods of solution exist in principle but of which utilization is not now practicable.

Practicability is a relative factor. For example, a computer simulation involving a few thousand variables (the type it takes to simulate detailed operation of organizations as small as battalions in battle contact) programmed and warranted as an applicable model for decision purposes, now requires several hundred man-years of effort expended over a duration of 1 to 3 years. This simulation requires, furthermore, a running time of $\frac{1}{6}$ to 2 hr on a modern computer for one play only—one of literally thousands required to accomplish meaningful results. Not only does it cost, therefore, from $2 million to $10

million and take 1 to 3 years to develop, but once developed it is also relatively rigid, so that changes desired will require costs and time amounting to an appreciable fraction of the first costs. And unfortunately, the decision problem may change appreciably in the time consumed in the building of this model.

In spite of its cost and time such simulation is currently practicable for a very limited class of decision problems—i.e., that involving production and operating costs of a single class of weapons at the $1 billion level. The goal of development of simulation techniques should be to make simulation practicable for a much wider range of decision problems. To do so, simulations of the size just discussed should be developable over a period of a few weeks (a speedup of a factor of 100 to 500) and at the cost of 2 to 10 man-years of effort (an increase of efficiency in programming of a factor from 50 to 200). Cost would then be about $50,000 to $200,000, and simulation would constitute a practical tool for improvement of decisions in the $1 million class. Developments now appearing in the computer sciences give promise of the ultimate achievement of this goal.

Conceptualization Needs

On the other hand, from a certain viewpoint some kinds of simulations should be designed more for the operations analyst than for his client. The replication of runs in a simulation can still be time consuming; furthermore it generates too much information (a complete one-to-one simulation of an operation would be just as difficult to analyze and understand as would the operation itself). The goal should not be ultradetail in simulation; rather, it should be used as a simulated source of experience for the analyst, leading to an intuitive conceptualization of a macromodel that contains a refactorization of concepts pertinent to a limited class of decisions faced by the client. The present attempt to develop a microsimulation to cover realistically all the decision areas in a complex organization is painfully inept, ludicrously ponderous, and overwhelmingly inundated by a diarrhea of output. To make an analogy, it is as if all the decisions pertaining to design, development, and operation of a steam power plant were based on a microsimulation of the motion of individual classes of molecules and waves in crystalline lattices. Without a more sophisticated and highly reduced model of heat propagation and without the new concepts of thermodynamics, such a class of decisions would be unattainable on a warranted scientific basis. There have been instances in past studies at RAC where several hundred hours of computer experience with a highly detailed microsimulation led to the formulation of very simple, sophisticated, and elegant decision models for which the computational requirements could be met by a few hours at a desk computer. These macromodels produced results applicable over a much wider range than was available with their corresponding micromodels and led to insights not attainable by the simulations. Yet it is doubtful that the simple macromodels would have been attained had not the experience with the simulations been undertaken.

This is an isolated example of an experience that can and should be commonplace. The technique of conceptualization of such emergent simplified (reduced) models is poorly understood at present.

The need for the development of this technique underlies present inadequacies in the area of battle models (see Table 1, Problem Area 9). Lanchester* in the period of World War I made a significant move in this direction. Yet attempts to apply Lanchester's concepts have for the most part been simplistic and unimaginative. The need here is for conceptual progress along the lines of generalized decision models (in the sense of generalized phase-space momentum, Lagrangian and Hamiltonian operators, etc., in physics).

Problems of Practicability

The choice between microdetailed model and reduced macromodel is very pertinent to two most important types of operations research problems: measures of effectiveness and determination of costs. Problems of effectiveness and cost determinations in military areas are particularly difficult for two reasons: (a) effectiveness and cost are not in general separable concepts, except possibly in situations where the values of the system under study are affected marginally, and (b) effectiveness and cost are relative quantities and can be particularized only in a specified context. Data on effectiveness and cost are rarely obtained in the context in which it is desired to apply them. Extrapolation from one context to another constitutes the central technological problem.

Such problems in principle can be solved by devising a micromodel in which, for a measurement of effectiveness, analyses of all conceivable situations are assessed and a model in which, for cost analysis, all the possible uses of each commodity are included. In such circumstances the evaluation of specific cost and effectiveness can be bypassed—or obtained by implication.

However, such a solution gives rise to what has been dubbed the "Number Barrier," illustrated in Fig. 1. The ideas were built on a lecture by W. Ross Ashby.† One is concerned here with the number of elemental mathematical (arithmetic) operations involved in the computation of a single answer; the number is plotted on a rough log-log scale. Figure 1 shows that numbers of operations ranging from 10^1 to 10^{15} might be termed the "computational range"; from 10^{15} to 10^{100}, the "astronomical range"; and from 10^{100} up, the "combinatorial range." Ashby has pointed out that simple simulation involving all the details of men, weapons, and terrain of a battalion-sized unit can, in principle, be loaded on a computer that is big enough. However, anyone committed to finding the best tactic by the simple-minded procedure of investigating all the possible histories of engagements has selected a program involving calculations numbering in the combinatorial range, e.g., 10^{500}—which, as Ashby says in notable understatement, is a very large number indeed. (The number 10^{100} represents a true barrier, since it is inconceivable that one could even approach this number of calculations—let alone exceed it.) Obviously this is not the way to solve problems. A present-day computer can produce 10^9 calculations/hr. The very fastest computer foreseeable in the future of hardware technology may produce about 10^{15} calculations/hr. The gap between 10^9 or 10^{15} and 10^{500} must be overcome by advances in methodology.

The number barrier can be overcome by applying these principles: (a) reduction in scope of decision problems accepted, (b) creative objectification

*F. W. Lanchester, Aircraft and Warfare, Constable & Co., London, 1916, Chap 4.
†W. Ross Ashby, paper presented to Second Symposium in General Systems Theory, Case Institute of Technology, Apr 63.

of macroconcepts, and (e) the importance methodology of the prescriptive mode of inquiry.

Importance methodology is a strategy of search for a solution (among the combinatorial possibilities) that utilizes experience gained by a current strategy to improve the strategy for the next move, so that the strategy becomes more efficient as the goal is approached. The number of computations ensuring an

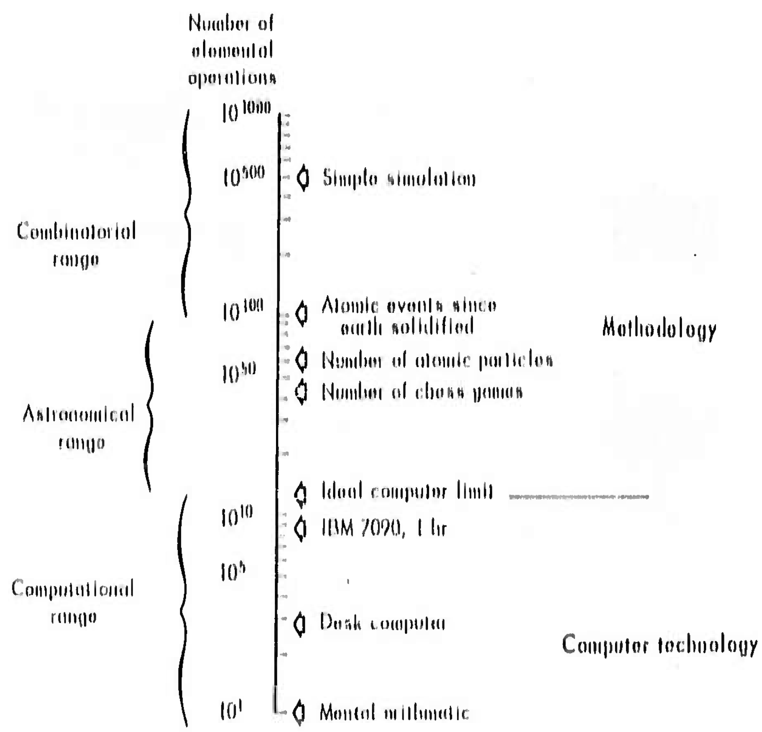

Fig. 1—The Number Barrier in Elemental Mathematical Operations Involved in Computing a Single Answer
(after W. Ross Ashby)

optimal solution can, in principle, be reduced to essentially the logarithm of the total combinatorial possibilities. If this is not possible, a strategy of calculation may be pursued that guarantees only an improvement over the last known solution and that may have a finite chance to find an optimal solution. Such a strategy is called "heuristic."

TECHNIQUES APPLICABLE TO PROBLEM AREAS

Consider now the list of problem areas of Army command and management (see the section so titled) in terms of the techniques applicable to their solution. These are listed in Table 1, together with identification of applicable major techniques and an estimate of present status of practicability, if any.

TABLE 1

Major Techniques Applicable to Problem Areas and Status of Conceptualization and Practicability

Problem area	Applicable technique	Concepts	Practicability
1a. Allocation of military resources	Linear programming	Available	Up to ~ 2000 variables
	Nonlinear programming	Available	Up to several hundred variables
	Stochastic programming	Available	Up to a few dozen states or 10 decision alternatives
	Integer programming, linear	Available	Up to a few dozen variables
	Integer programming, nonlinear	Not available	Not now practicable
	Dynamic programming	Available	Up to a few decision alternatives in a few dozen states
	Combinatorial restrained programming; traveling-salesman problem	Available	Limited by computing equipment; recent algorithm gives high probability of efficient, exact program up to 100 variables
	Logistics theory	Available	Problems of moderate complexity
	Input-output analysis	Available	Up to ~ 2000 variables, data limited
	Graph theory	Elemental theory	Limited
	Queuing theory	Fairly well developed	Exact solutions limited by computing equipment; practicable for steady-state solutions, stochastic solutions limited to a few dozen variables
1b. Military costs	Military economics	Marginal analysis well developed	Limited by availability of data and by lack of theory permitting application to new areas
	Simulation	(see 3)	(see 3)
	Value theory	Basic concepts developed; exploitation of theory desirable	Applied to allocation methods and value measures in isolated instances
2. R&D management	Decision under uncertainty, value theory	Available, but practical interpretation difficult	Now limited
3. Measures of military effectiveness	Value theory, simulation and gaming	Foundations developed	Limited by data, computer, and computer programming
	Cognitive theory	Formulated	Application not well developed
	Organization theory	Isolated concepts emerging	Application not well developed
	Systems theory	Isolated concepts emerging	Not now established

TABLE I (continued)

Problem area	Applicable technique	Concepts	Practicability
	Command and communications theory	Special concepts developed	Practicable in some fields
	Information retrieval, data synthesis, pattern recognition	Needed	Not now established
	Game theory	Available	Not now practicable, except as guide to methodology
4. Projection of innovation into future battlefield	(Presupposes all those techniques involved in measures of effectiveness; all techniques involved in battle models involve high level of complication)	Limited where available, consist only to detailed (microsimulation)	No systematic method available under complex conditions; few simple battle conditions projected, otherwise largely "thinkpiece" projections
5. Production of meaningful and applicable data for measures of military effectiveness and tests of innovations	Design of military experiments	To some degree, limited; incompletely implemented	Very limited as to productivity and time, reaction of environment to experiment neither understood nor under control
6. General desideratum (military operations research methodology)	Design of admissible, meaningful, and practicable model leading to decision under uncertainty	Some progress, primarily, conceptually limited	Practicable in limited application
7. General desideratum (operations research methodology)	Increase in comprehensiveness and realistic detail	Simulation conceptually limited	Limited by computer and computer programming
8. Automatic data processing and automated intelligence (also includes cognitive theory, systems theory, pattern recognition)	Method of processing and retrieval of information	Limited	Not now practicable
9. Interaction of logistic and tactical factors	More general battlefield problem	← Either impracticable concepts available or conceptually limited →	
10. Management policy	Value theory, organization theory, cognitive theory	Only foundation concepts available	Not yet practicable; applicability of simulation limited
11. Warrantable macromodels of combat	Cognitive theory and theory of optimization	Limited, require major synthesis	Practicable macromodels attained in isolated instances

FORECASTS OF TECHNICAL DEVELOPMENTS

General Predictions

Strong intimations currently may be discerned that operations research and its allied fields (management science, cybernetics, general systems theory, behavioral science, computer sciences, and modern mathematics of optimization) are converging toward a common methodological and intellectual innovation—an innovation expected to accelerate greatly the rate of generation of new warrantable and applicable theory, i.e., theory or models of the type needed to produce macromodels that are practicable for the solution of problems. The innovation amounts to a systematic process for the control and modification of the strategy of theorizing. At the present time this strategy is applied by each investigator intuitively and covertly, but ways are beginning to be found of so devising theory that it will correct and perfect itself in the context of a set of more general concepts and norms. In such a context the strategy of perfection of the theory becomes an overt part of the process. Perfection of metatheory of this type frees the investigator to apply his intellectual energies on a more general level by making routinely applied acts of operations that once totally occupied his attention. The method, termed a "normative" or "prescriptive" mode of inquiry, includes and supplants previously developed methods of logic, empiricism, and objectivism. The terms "general systems aspect" in general systems theory, and "heuristic programming" in the computer sciences identify the emergent innovation, which also relates to the principle of optimality of dynamic programming.

It is predicted that the trend described here toward utilization of prescriptive inquiry will be significantly developed during the next 20 years and that the disciplines cited will become completely overlapped. A new epoch can be foreseen, in which the great cultural advances will be made in prescriptive fields. That epoch will gradually replace technology as the most fruitful area for progress. Although as many as 50 years may be required for the maturation of this epoch, the signs of its coming should be apparent to all in as few as 20 years. The science of management, for its part, will benefit directly, and the innovations appearing in management will outweigh all other innovations of technology.

As a corollary to this opinion, the support of R&D in all the fields named above will contribute toward the attainment of this achievement, and the time scale of achievement will naturally depend on the level of support.

Advances in Techniques of Operations Research

Decision Algorithms. The main advances in methodological research on improving techniques of operations research in the short range (0 to 4 years) will continue to be in the general area of decision algorithms. The development and enlargement of techniques of nonlinear methodology, particularly nonlinear programming (allocation of resources in which utility and restraint functions are nonlinear), will be heavily exploited. Graph theory, now utilized in a rudimentary manner by the Program Evaluation Review Technique (PERT) management system, will attain notable enlargement, and its basic foundations will be laid for later exploitation.

The use of input-output models will increase because of advances in the collection and interpretation of data, which will illuminate the inadequacies of present input-output modes; and the availability of better data will mak worth while the formulation of much more sophisticated economic mode

There will be an increased use of integer programming—both linear and nonlinear. Stochastic programming will be successfully applied to a large class of problems. Means will be found for practical approximate solution in models having on the order of 1000 states.

Combinatorial programming will be advanced conceptually and means will be found for closed solution of a rudimentary set of practical problems.

Dynamic programming will continue to be developed, although its full maturity is not forecast until later time periods. The inclusion of spatial (linear and nonlinear) stochastic and dynamic programming will be achieved for relatively simple models.

Midrange (4 to 8 years) progress will proceed along these same lines. The lifetime of exploitation of any one technique seems to be on the order of 7 to 10 years, so that great changes are not expected until nearly the end of this period, which it is thought will be characterized by extensions of existing techniques to take advantage of progress in the computing arts. Linear and nonlinear programming for problems on the order of 5000 to 10,000 variables will be practical, but the advantages will be of the marginal-improvement kind. The primary conceptual advancement will be in the general field of combinatorial programming (graph theory plus programming). Computational algorithms for spatial-dynamic programs will appear, and practical dynamic programs involving 10 to 100 spatial variables will be developed.

Heuristic algorithms will become more commonplace. (These algorithms do not necessarily find the optimal solution in closed form; they will always find better solutions, however, and they may have a finite probability of finding the exact solution.)

Long-range and *very long-range* (8 to 20 years) progress in the development of decision algorithms will be strongly influenced by the progress in two other areas: the computer sciences and information-retrieval pattern recognition. The innovations constituting notable advance will be the appearance of decision algorithms that make use of the principles of pattern recognition in the process of improving their strategies for convergence to the solution. This kind of problem solving will be under development and exploitation for many subsequent years. The forecast here is that it will make its first practical appearance in the 8-to-20-year period.

Value and Policy Determination. In the *short-range* period, research in value theory will be accelerated and the foundations of value theory will be established. Operations research on policy determination will begin to appear. Macromodels for economic and value measures will be used to a greater extent. Although the trend of development of marginal analysis will continue in economics, more attention will be given to holistic theories—following the focus of attention on these theories given by value theory.

Midrange and long-range progress will consist of the development of practical operations research methodology in the realm of policy and in areas where management goals and strategies are not fully determinable. Such areas

are management of research, investment programs, disbursement of charity and welfare funds, community projects, education, and advertising. Value science—the science of measuring the values held by a system as an object—will become more highly developed. Such development will contribute to the techniques of determining numerical measures of effectiveness. In the very long-range period policy research will develop to an appreciable degree. (Such research constitutes a service to an institution paralleling the services of a psychiatrist to an individual.)

Organizational and Systemic Measures and Techniques. Advances of techniques will be carried out on several fronts: cybernetics, general systems theory, behavioral science, and the computer sciences, as well as the management sciences. An increasing amount of conceptual research will be focused on information retrieval and pattern recognition. Short-range advances will consist of development of theory and specialized hardware that can "read" various selected typefaces and recognize words. Midrange and long-range exploitation of this technique will be the appearance of special machines that can scan newspapers or other print for patterns of words and detect any significant changes in these patterns. Very long-range development may include the output of several pattern-searching machines of different types (print, sound, radar echo, etc.) into one composite "intelligence" machine. These machines will be rudimentary, compared with man's ability—but they may be practical in the filtering of large quantities of low-grade information, e.g., the frequency of occurrence of words in a foreign newspaper or words uttered over the radio.

The organization will be looked on more and more as an organism. Measures of the characteristic systems response will be perfected so that its viability with respect to solving different classes of problematic situations will be predictable. The man-machine aspects, particularly in the design of communication nets and information filters, will be subject to successful application of operations research.

Machine language translation, in a specialized sense, appears practical in the midrange period if supported now. This presupposes the invention of, say, a specialized military language and military vocabulary—along lines similar to nautical flag signals and radio international codes. Machines of this type would be particularly useful in situations where representatives of several nations are engaged in joint military operations.

Area of Decision Models. The short-range period will see the foundations of the prescriptive mode of theory appear. These fundamentals will gradually diffuse into use in the practical activities of operations research scientists, and will be exploited in the midrange and long-range periods.

Simulation (and computer gaming) will obtain impetus from developments in computer hardware and computer programming. A computer simulation hyperlanguage has appeared this year, but it is too early to assess its utility. Simulation has been late in having a hyperlanguage available for it. As a result, simulation programming has been done in computer language—the most elementary and slowest way to program. Some special simulation languages have been used, but, by design, had too narrow an application to be generally useful. The short-range period will see the emergence of generalized simulation computer language, which will result in a speedup of programming by a factor of

10. This capability will be further enhanced in the midrange period with the perfection of simulation computer languages resulting in a speedup by a factor of about 100.

Increased use for simulation purposes of specialized computer ancillary equipment will occur. This special equipment will be used to speed up and translate the input-output communication between the analyst and the computer. Equipment of this nature is now available, but its cost—and the cost of programming support needed to exploit it—has been so relatively high that little use has developed so far. This type of equipment will become commonplace by the midrange period.

The increased use of simulation will strengthen the need for reduced macromodels; the operation of the simulation will constitute a source of artificial experience that will lead to aggregated concepts. Studies of generalized theory will accelerate this process. The emergence of unifying concepts in macromodels of military forces will result. This will be a slow process and will begin to be noticed as a normal procedure in the very long-range period.

Computers and Computer Programming

Since the development of computers and computer programming determines to a high degree the development of other techniques, it is appropriate to give special attention to this field. At present hardware developments aimed at faster and more reliable elements are emphasized. Hardware developments have outstripped the computer programming development by 5 to 7 years. In terms of the average "generation lifetime" of a computer design, programming is then a generation—at least—behind hardware. Thus specifications of hardware are determined more by engineers than by the programmers who use the machines. The race for faster machines is approaching the point where the spatial dimensions of the machine will affect the time phasing of its operation (because of the finite speed of electromagnetic pulses). When this condition is reached—operation times on the order of the size of the machine divided by the speed of light, or 300 cm/(3×10^{10} cm/sec) $\sim 10^{-8}$ sec—the technology will have to look to some other means of speeding up computational processes.

The next 10 to 12 years (short- and midrange periods) should see the appearance of half-microsecond (50 times the limit given in the preceding paragraph—which is not a distinct go-no-go limit) memory as fairly standard. Such a memory size will contain several 32,000 modules—and the cost of operating will go up. Special-register, multicontrol, and polymorphic machines will also appear in greater numbers, although these features will not be widely exploited—because the programming art is lagging. Such machines will be applied to large-scale production and real-time data processing. The real-time data processing will provide most of the impetus for the development of machines with more parallel processing (polymorphic). This area has been well funded because of the heavy expenditures in our space program.

In general, these machines will be designed for the hardware needs of space tracking systems. Although they will represent developments of outstanding quality, they will not necessarily represent the ideal computing systems to satisfy Army requirements. Thus far the low input-output "scientific," (i.e., one-answer output) and the data processing (business operations) machines have

dominated the field. If the Army continues to allow all [illegible] m computers to be underwritten by other services and by indus[illegible] d continue to have to convert hand-me-down designs for its purposes and to obtain a poor fit to its requirements at best.

If the R&D is supported, the long-range and very long-range periods could see the appearance of high-speed, low-cost, small, high-capacity, reliable, polymorphic computers deriving these characteristics from microminiaturization—i.e., circuits and transistors made on printing-press-type machines. Because the individual element made by this process would have a low reliability (~ 90 percent) the circuits would have to be highly redundant. The level of redundancy should be such that the computer would function with 50 to 75 percent of its elements inoperative. The whole computer module would be expendable; when its reliability or functioning failed, it would be discarded. In concept the computer should be designed around the needs of the programmer, and in particular around the needs for programming and functioning of the pattern-recognition operation. Such a computer could serve the Army's needs in the many areas of data retrieval and intelligence synthesis with which it is concerned. The automation of data-handling operations in the command and control ladder would be greatly accelerated by such a development.

As a corollary, any device that contributes to these data-retrieval and intelligence-synthesis areas will also serve as a useful tool for the operations analyst, since he is seeking to perform these very same operations—or to learn how these operations may be performed—for the Army.

As the cost of general-purpose computer installations increases, more and more institutions will be priced out of computer operation and will have to depend more and more on large central computer-service organizations. These will become more automated, until by the long-range period the computer-processing service will be much like the telephone system. Each subscriber will be satellited onto the major computer by data links and peripheral input-output devices. The whole operation will be controlled by a master computer monitor that will receive, assign to a computer, store, process, correct some kinds of coding errors, improve programming codes, cost-account, transmit answers (and operating codes if asked), and bill the client.

The programming art in the last decade has shown a factor-of-100 increase in efficiency through the development of algebraic languages and compiling of hyperlanguages. This trend will be accelerated. The next decade will see the appearance of programs that improve themselves as they operate. Special programming developments for the field of pattern recognition will be exploited in the science of programming itself.

FORECAST ADVANCES IN ARMY PROBLEM AREAS

We shall now attempt to translate these technical advances into advances in the art of solving problems of direct interest to the Army by applying forecasts to Table 1. (Numbers refer to problem areas listed in Table 1.)

1. Allocation techniques will be generally advanced through the short-range and midrange periods. This is also true of military cost analysis except insofar as it depends on simulation. Since major advances in simulation are

not foreseen until nearly the beginning of the long-range period, militar
ing and military-effectiveness determination will lag behind the ability to
straightforward allocation problems (where costs and effectiveness are

2. R&D management problems in the short-range period must be co
sidered basic research; however, a concentrated attack on these problems
result in a more satisfactory method of operation in the midrange period.

3. Measures of military effectiveness will be limited for a period by
in the development of simulation as a flexible and versatile tool. Progress
problems in this category also strongly depends on the appearance of goo
battlefield models, which—although not the subject of simulation—will be
accelerated by the availability of flexible simulation and, as a source of
"experience," will lend to creative intuition. Ability in this area will be
erated and will come into maturity in the very long-range period.

4. Projection of innovations into future battlefields will continue to
one step behind development of good battle models.

5. The production of applicable data will depend on the existence of
able models for extrapolation. Both increased use of field experiments to
erate data for simulation inputs and changes in the concept of experiment
design will yield tremendous improvement in the analysis of Army operat

6. Models for decision under uncertainty will represent a methodol
development. Decision under uncertain data or in poorly known condition
ordinary rather than unique. Our present technique—treating a projection
years in the future with the same detail as immediate forecasts and usin
detailed and precise probabilities of outcomes—is not meaningful. The
used in such projected studies must reflect the inherent uncertainty.
concepts for such a representation are known. Practical progress in
will be made in the short-range period and develop maturity in the mid
period.

7. Increase in comprehensiveness and realistic detail will depend
provement in techniques of experimental measurement and simulation.
ment in simulation depends, furthermore, almost wholly on simulation
programming development. Significant advances in experiment and simu
in the short-range period and major advance in the midrange period are

8. Automatic data processing and automated intelligence can show
standing progress in the long-range period if sufficiently funded. Advan
in this field depend largely on advances in data retrieval and in pattern
nition, as well as on advances in the computer-programming art.

9. The capability to consider interaction of logistic and tactical fa
through development of more comprehensive military models is not ind
of the areas already discussed. Advancement will depend on advances
lation and in macromodel formulation. The forecast is for accelerating
but no major progress until the long-range period.

10. Management policy analysis will be developed by some investi
and significant progress will be achieved in the midrange period.

11. The development of macromodels for military and other uses
discussed earlier in these forecasts (see "General Predictions" secti
though the frequency of appearance of such models will accelerate, the
novations in this area are not expected until the long- and very long-ra